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OBJECTIVE PRISM RADIAL VELOCITIES

By PETER M. MILLMAN

AS the determination of radial velocities of all stars brighter than magnitude 6.0 nears completion, and as larger numbers of velocities of stars down to 8.5 magnitude are determined, it becomes increasingly clear that the present methods of velocity determination are far too time-consuming to make possible even a partially complete survey of the fainter stars (9.0^m-11.0^m) within the next three or four decades. Even with a large telescope, like the 72-inch at Victoria, and the shortest focus camera (dispersion 90 A to the mm) to photograph a ninth magnitude star requires an exposure of from one to three hours, depending on observing conditions. With the average amount of fine weather this would mean about 250 individual radial velocities of tenth magnitude stars a year, on the assumption that the telescope was used for only this one problem.

And yet radial velocities of faint stars are statistically very important. They are needed for the determination of the solar motion with respect to these stars, which quantity has many uses such as the reduction of secular parallaxes to annual ones. Then the radial velocities of faint stars would assist greatly in the study of galactic rotation, no other material serving quite the same purpose. When used statistically, radial velocities need not have probable errors less than the mean random motions of the stars considered, so that large numbers of velocities with probable errors as great as 15 kilometers per second might be very useful in some cases.

Under these circumstances it is natural to look to the objective prism as a possible solution of the difficulty. This instrument

utilizes a much greater proportion of the incident light than the slit spectrograph, and in addition to this it photographs large numbers of spectra in one exposure. The chief disadvantages of the objective prism are the difficulty of finding a standard from which to measure absolute wave lengths, and the very low dispersions with which one usually has to deal. E. C. Pickering in 1887[1] was the first to suggest the possibility of using the objective prism in conjunction with an absorbing medium for the determination of radial velocities, and since then a number of astronomers have published papers on the subject without any very definite results having been obtained. It is the purpose of the present paper to review briefly the methods suggested up to the present time, and to summarize the chief results obtained by their use.

The methods which have been suggested for obtaining radial velocities with the objective prism (or grating) may be classed under four heads:

1. Use of standard bands given by an absorbing medium.
2. Methods depending on the relative displacement of two mutually reversed spectra for each star.
3. Measurement of change in length of spectrum.
4. Miscellaneous methods.

We shall now consider the suggested methods in the above order:

1. *Absorption Standard.*—As noted above, E. C. Pickering was the first to conduct experiments along this line. In 1887[1] he suggested the use of hyponitric fumes or of the earth's atmosphere as the absorbing medium, and in 1891[2] mentioned dydymium. None of these suggestions was found practical however. W. W. Campbell in 1895[5] and Hale and Wadsworth the following year[11] again made the suggestion of using telluric lines, especially the B band, but no practical tests were made. No further advance occurred until 1910 when R. W. Wood[21, 22] conducted a series of experiments with various substances in an endeavour to find sharp absorption bands for use as standards. An aqueous solution of neodymium chloride was found to give a sharp band at $\lambda 4272.90$ width only three angstroms, as well as several other less well-defined bands in the neighbourhood of $H\beta$. The wave length given above was found to remain constant over an appreciable range of temperature. The only other substance Wood seriously suggested was peroxide of chlorine[23]. The heads of the bands of this

compound are very well determined but the bands themselves hide too much of the stellar spectrum. To this date neodymium chloride has proved the best suggestion as an absorption standard.

2. *Reversion Method.*—The principle of this method consists essentially in obtaining two photographs of the same star field, either on the same or on separate plates, the spectra in the second exposure being reversed with respect to those of the first, *e.g.*, if one exposure had blue north, the other would have blue south. If the two spectra for each star are placed side by side, it is possible to measure the relative shifts of the various pairs of lines, which will amount to just twice the shift for a single spectrum. It is obvious that this method will determine only the *relative* radial velocities of the stars on the plate.

The reversion method was first outlined by Hale and Wadsworth in 1896[11]. They suggested four possible arrangements. The simplest was to rotate the prism through 180° between exposures. Absolute measurements could be made possible by photographing star images simultaneously through an aperture in the central part of the objective prism, a small doubly reflecting prism giving the same deviation for the star images as was given by the prism for the spectral images. It was also suggested that changes in dispersion due to temperature variation and flexure could be checked by photographing a terrestrial source while utilizing the full aperture of the objective prism. This could most conveniently be accomplished with an adaptation of the Littrow spectroscope where the objective acts as the collimating lens.

The second suggestion was to use a direct vision objective prism to reduce errors due to change of flexure. Another method whereby the telescope would not have to be reset between exposures would be to utilize some reversing arrangement, such as one formed of three plane surfaces. Finally this reversing apparatus could be made to cover only half the objective and thus both photographs could be made at the same time. The difficulty in the last three methods is the great loss of light occasioned, and the loss of definition due to imperfections in the many extra refracting and reflecting surfaces. E. C. Pickering later in the same year[12] discussed the reversion method, but recommended reversing the whole telescope between exposures, thus leaving the prism fixed with respect to the objective. Later, in 1906[17] instead of using two

plates and superposing them when measured, both exposures were made on the same plate. In 1906 Comstock[16] suggested two direct vision prisms each covering half the objective, but here again loss of light is the chief objection to the method. L. Stewart[18] designed an instrument composed of two objective prisms and two objectives so placed that both gave images on the same plate. At the best this would give practical results only for one star directly on the axis, as the plate must be tilted with respect to each objective. Schlesinger in 1909[19] suggested using two similar telescopes of moderate aperture but enclosing the whole in a constant temperature case, thus eliminating errors due to temperature change in the prisms. He also advised the use of plate glass photographic plates. One of these would have to be placed in the instrument with its glass side to the front, so that the two plates could be measured film to film. In 1913[27] Schlesinger mentioned the possibility of using a prism of smaller aperture than the objective by placing it between the objective and the eyepiece and using a small diverging lens to render the light parallel.

By far the most complete discussion and test of the reversion method was made by Schwarzschild in 1913[24]. He used essentially the same method as did Pickering in 1906, that is, he reversed the telescope and plate holder between exposures and used the same plate throughout. He discussed the matter of plate errors and adjustment errors very completely and developed all the necessary correcting formulae. Absolute velocities were found in two ways. The first was to make both exposures through the neodymium chloride screen, mentioned above, and use measures made on the line λ4273 to determine the plate constants. The other was to use for this purpose standard velocities of three stars on the plate. These velocities were assumed to be accurately known from results with the slit spectrograph. Variations in the velocity of each star individually were determined by placing two such plates on a stereocomparator, an instrument which superposed images of two plates in the same field. The plates were so adjusted that corresponding spectra from each plate lay next each other in the field of view, and so variations in relative shift were easily measured. Three variations of the reversion method were suggested in 1930 by N. Deisch[32]. The first two of these are identical

with ones mentioned above, the third, employing two trains of prisms, is quite impractical because of the large absorption of light and various mechanical difficulties.

3. *Length of Spectrum.*—This method is based on the fact that for any spectrum the shift in wave length caused by a specified radial velocity will vary for different parts of the spectrum thus causing a change in length of the spectrum. E. C. Pickering in 1891[2] was the first to mention this method, but stated that the resultant change in length would be so small that it would become confused with changes due to temperature. Other astronomers later suggested the same method with slight modifications. Orbinsky[4] advised photographing a standard star spectrum next to each spectrum whose velocity was to be determined. Frost[6] considered the use of one standard star on each plate. Deslandres[7], following Orbinsky, suggested the further refinement of photographing a terrestrial source simultaneously with each star spectrum. This would provide a check on changes due to temperature but is open to objection, as pointed out by Keeler[10], in that the optical system for photographing the terrestrial standard has a very different resolving power from that used for photographing the stars.

A somewhat analogous way of obtaining the radial velocities of the stars would be the use of an objective grating. Here the distances apart of the various orders of spectrum, as well as change in length of spectrum, could be measured. Methods of constructing an objective grating were outlined by Jewell[3] and Hale and Wadsworth[8, 11] over thirty years ago. Hamy in 1913[25] gave the theory necessary for obtaining radial velocities from two successive orders of spectrum given by a plane reflection grating when photographed along with a terrestrial comparison source.

The chief difficulties in the practical application of all the above methods are the very small change in length of the spectrum relative to the total length, and in the case of the grating, the great loss of light which makes it impossible to use it for faint stars (the chief field for objective prism work). The most hopeful suggestion along this line was made by Schlesinger in 1913[27]. His plan was to use a specially constructed lens (such as the Cooke triplet) which would bring into good focus two widely separated regions

of the spectrum λ3900-λ4500 and λ5900-λ6600. Due to small dispersion in the red the Doppler shift in the latter region would be practically zero and the measured change in length of the spectrum would consist almost entirely of the shift in the violet region. Greater accuracy would be insured by enclosing the whole instrument in a constant temperature case.

4. *Miscellaneous Methods.*—E. C. Pickering as early as 1891[2] recorded various attempts that were made to utilize the star images photographed through an auxiliary achromatic prism as standard points from which to measure the relative shifts of the spectrum lines. None of these methods proved practical. Maunder in 1896[9] also suggested star images obtained with a second telescope, a second exposure, or the outer part of the objective. Hale and Wadsworth[11] in the same year mentioned the possibility of using the interference bands produced by a pair of parallel plates placed either in front of or behind the prism. Finally Hamy, in 1914[28] outlined a rather complicated arrangement utilizing two plates rigidly fixed to each other, one to photograph the spectra and the other to photograph direct images. In addition, a standard terrestrial source was to be photographed on both plates. All the above methods would be very difficult to apply in practice, chiefly because of the elaborate nature of the set-up, making possible a multitude of instrumental errors which might entirely mask the extremely small shifts arising from radial velocities.

5. *Summary of Results Obtained.*—Of all the suggested methods, actual measures have been published for three, the Pickering reversion method, the neodymium absorption method, and the length of spectrum method.

The only results obtained with the reversion method were published by Schwarzschild in 1913[24]. He used an ultraviolet Zeiss triplet of 5.9 inches aperture and focal ratio 1:10. This, when coupled with a prism of 50° refracting angle, gave a dispersion of 5.6 mm. from Hγ to Hδ*. The procedure suggested by Pickering was followed: that is, the first exposure was made, the telescope and plate holder were then reversed and the second exposure made,

*Schwarzschild throughout his paper tabulates a value just twice the true dispersion as this is effectively what is measured in the reversion method.

the two exposures being of equal length. Guiding was accomplished by a telescope with a reflecting prism attachment which gave the same deviation as the objective prism. Three plates were taken of the Hyades (center at $4^h\ 23^m$, $+15°.7$. Plate I, with the neodymium screen, was given exposures of about 30 minutes each, plates II and III, without the screen, exposures of about 20 minutes each. Radial velocities referred to the sun for seventeen stars (mean photographic magnitude 5.0) were obtained from the first two plates. For plate I the three plate constants were determined by measures made on the neodymium line λ4273. For plate II these constants were found by using standard velocities of three of the stars as found by the slit spectrograph. In all cases the adjustment errors were computed using a chart plate of the same region. On the average nine lines for each star were measured. Two plates centered on α Cygni were also taken, the neodymium screen being used, and two 60-minute exposures being made on each plate. 15 stars (mean photographic magnitude 5.8) were measured. The plate constants were derived from measures of the neodymium line λ4273. On the average five lines were measured in each star.

Mean probable errors for a single velocity determination were computed from the interagreement of the individual line measures; they are given below*:

PROBABLE ERRORS OF A SINGLE VELOCITY

Plate	*Hyades Region*			*Cygnus Region*
	5 stars Mean class K0 Mean pg. mag. 4.5	8 stars Mean class A5 Mean pg. mag. 4.9	4 stars Mean class A5 Mean pg. mag. 5.8	15 stars Mean class A0 Mean pg. mag. 5.8
	km/sec	km/sec	km/sec	km/sec
I	±2	±3	±5	±6
II	±3	±4	±7	±4

*These do not exactly correspond to the probable errors given in the original paper. On checking his values it seems that Schwarzschild tabulated the mean error and called it the probable error. I have recomputed the probable errors from his measures using the standard formula.

From the above it would seem that the lines for each star are in slightly better agreement when the neodymium line is used for determining the plate constants. Schwarzschild, however, finds that the absolute velocities of the Hyades obtained from plate I with the help of the neodymium line show a systematic difference of −26 km as compared with those obtained from plate II using three standard stars. He says that this is possibly due to the fact that the line 4273 is at one end of his spectra and hence poorly defined.

Velocities for the Hyades plate I were recomputed using the three standard stars. Plates II and III of the Hyades were also compared directly with plate I by means of the stereocomparator mentioned above. Thus in all three determinations of the velocity of each of the stars in the Hyades region were made and two for each star of the Cygnus region. Assuming constant velocities for all the stars this gave mean probable errors as below:

14 Hyades stars..... ±7 km/sec
15 Cygnus stars..... ±7 km/sec

These agree well with probable errors derived from interagreement of the lines.

The only recorded measures of radial velocity from the length of spectrum method were made by J. S. Plaskett in 1914[29]. He used a 4½-inch Cooke Photo-Visual telescope with two objective prisms of 15° and 25° refracting angle. This combination gave a linear dispersion of 7.8 mm from Hδ to Hγ. Three pairs of spectra were taken, the stars used being αTauri and αOrionis. Lines in the range 5900A-3900A were measured. The differences in velocity for the two stars were computed from measures on the distances between lines near the opposite extremes of the spectra. The deviations of these velocity differences from those determined from slit spectrograph velocities were +2.1, −13.2, −2.9, indicating a probable error for an individual velocity of about ±6.5 km/sec. Plaskett remarks that without temperature control for the prisms, spectra obtained off the axis are not to be trusted, and this limits the method to the determination of but one velocity with each exposure. In addition, a bright comparison star of very similar type and standard velocity must be photographed on the same plate before and after each exposure. For equal dispersion the

gain in light of this objective prism arrangement over the slit spectrograph was about 80 per cent, hardly enough to compensate for the greatly decreased accuracy. Plaskett also suggested the possibility of using a slit spectrograph with a very wide slit, this having a light efficiency more nearly equivalent to that of an objective prism.

Another method that has been used in determining radial velocities from objective prism plates is to assume the wave length of the neodymium absorption band to be accurately known and to measure the displacement of the other lines with respect to it. Wood photographed this band with a 21-foot concave grating in 1910[23] and obtained the value 4272.90 angstroms, which is 4272.77 in the international scale. The width of the band is about three angstroms.

Pickering the same year[20] published results of preliminary settings on lines in five stars photographed with the 11-inch Draper telescope, giving a dispersion of 1.6 mm Hγ to Hδ. He found probable errors of the settings as follows:

H	±10 km/sec
H	±11 "
Nd	± 7 "

The next work was done by Schwarzschild in 1913[26] using the same instrument as was used in determining radial velocities by the reversion method. The dispersion was 5.6 mm from Hγ to Hδ as before and all plates were taken with the neodymium screen. Ninety-four spectra of the binary system α Coronae Borealis (class A0, pg. mag. 2.3) were obtained with exposures of 5 minutes each. The mean of all the measures was taken as representing the mean dispersion, considered invariable, and from measures of Hγ, Hδ, Hε and K, the velocity of each plate was computed relative to the mean of all the plates. The constant factor for changing these relative velocities to absolute velocities may be obtained from the known wave length of the neodymium line. Schwarzschild, however, reversed the process and found the average difference between his relative velocities and the values given by Jordan's elements. This correction gave a value of 4272.66 I.A. as the wave length of the neodymium line. Lindstedt obtained the value 4272.64 I.A. from laboratory measures. The

final deviations from Jordan's elements gave a probable error for a single plate of ±13 km/sec.

In 1918 Graham[20] published the results of measures made on a plate taken with the neodymium screen and the 16-inch Metcalf doublet of the Harvard Observatory. The 6° prism used gives a dispersion of 1.2 mm from Hγ to Hδ. The plate was centered at $12^h\ 36^m$, +40°.5 and exposed for 60 minutes. It is of excellent quality, and twenty stars of mean photographic magnitude 8.0 were measured. The hydrogen lines of the ten best stars were used to determine a mean dispersion, which was then adopted as the standard. In the computations the mean velocity of these ten stars was assumed zero and then from the wave length of the neodymium line computed on this assumption a correction was found which was later applied to the star velocities. The measures for the stellar lines of each star were so corrected that their distances apart corresponded to the standard dispersion and then the position of the neodymium line gave a measure of the velocity of the star. The results as Graham records them are somewhat misleading; it seems as though each line gave him an independent determination of the velocity for a star, whereas in reality the stellar lines were artificially made to show the same velocity displacement by his method of reducing the measures to the standard dispersion. Hence there is only one independent determination of velocity for each star and no probable errors can be computed from the inter-agreement of his individual line measures. The complete process of measuring was repeated by Graham four times on the same plate and using the agreement of these four sets to compute probable errors (a proceeding of rather doubtful value) I find a mean probable error for one plate of 6 km/sec. This is remarkably low for the dispersion used but it is not very indicative of the actual accuracy of the results since it is only derived from a repetition of measures.

Graham used the wave length 4272.80 for the neodymium line, while to conform with the other wave lengths given in the paper he should have used 4272.90 as found by Wood.[23] Another difficulty is that eight of the twenty stars measured are of classes G and K; in these late types the iron lines at 4271.5 are of appreciable strength and with low dispersion are completely blended with

4272.8. Hence there is likely to be a systematic error in measures of late type stars by this method.

Measures of radial velocities with the objective prism have also been published by H. C. Wilson.[31] He measured some half dozen plates of Praesepe, taken at Harvard with the same instrumental equipment as was the plate measured by Graham. No individual measures were published, but Wilson stated that for the 13 brightest stars in the cluster (mean pg. mag. about 7.0) the velocities on the best plate ranged from +77 to −65 km/sec. He considered the probable uncertainty of the measures to be ±10 km/sec.

Recently, at Harvard, the writer conducted an investigation to determine a more advantageous method of measuring spectra obtained with the neodymium chloride screen. An instrument similar to the Hartmann Spectrocomparator, but fitted to accommodate 8 ×10 inch plates, was constructed. In the field of view alternate strips of the spectrum of the star to be measured and of a standard spectrum could be observed, both spectra having been photographed with the neodymium chloride screen. Velocities referred to the standard star were determined by successively aligning the hydrogen lines and the neodymium bands. A full account of the investigation appears in the Harvard Circulars.[34] This method does not necessitate exact knowledge of the neodymium wave lengths and so four of the neodymium bands were utilized as velocity standards. Eight plates of average quality centered on the Cygnus region of faint early type stars were used, ten stars being selected for measurement on each plate. All observations were made with the 16-inch Metcalf doublet, dispersion Hδ to Hγ 1.2 mm.

Measures of the same material were also made on a Gaertner measuring machine, the velocities in this case being computed from the single neodymium band 4273, using the wave length given by Wood. Probable errors as computed from the interagreement of velocities found on the various plates were large but the comparator measures seemed to be distinctly superior to the Gaertner measures. This probably results from the use of a larger number of neodymium bands in the former case. The mean probable errors for a single observation of the seven stars well in from the

edge of the field, mean photographic magnitude 6.1, mean classA 0, were:

Comparator..... ±31 km/sec
Gaertner..... ±70 "

Taking all ten stars, mean photographic magnitude 6.4, we get

Comparator..... ±45 km/sec
Gaertner..... ±67 "

These errors render the velocities quite useless, but at least indicate that the comparator method of measurement is preferable to the measures of line positions with a cross wire. The above errors could probably be greatly reduced had plates of the highest quality been obtained. Unfortunately, owing to weather conditions, this is not always possible.

6. *Summary.*—The probable errors of the various velocity measures in km/sec are summarized in the accompanying table. The sixth column gives the average deviation of the objective prism radial velocities from those previously determined with the slit spectrograph. These are larger than might be expected, partly owing to the presence of a number of spectroscopic binaries with undetermined orbits. It will be seen that in general the brighter stars have smaller probable errors and that the correlation of

Method	No. of Stars	Mean pg. mag.	Effective Dispersion Hδ-Hγ (mm)	Probable error (km/sec)	Average Deviation from slit-spectroscope vel.	Authority
Length of Spectrum	2	2.2	7.8	± 6		J. S. Plaskett
Reversion	29	5.4	11.2	± 7	16 (21 stars)	Schwarzschild
Nd Absorption	1	2.3	5.6	±13		"
Nd Absorption Comparator	10	6.4	1.2	±45	36 (7 stars)	Millman
Gaertner				±67	32 (7 stars)	"

probable error with effective dispersion is rather definite. It seems likely that if velocities of faint stars are to be obtained with sufficiently small probable errors a telescope will have to be designed especially for this purpose. Preferably it should be adaptable to all three of the above methods, though we are least likely to meet with systematic errors if the method of absorption by neodymium chloride is used.

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THE METHOD OF ERATOSTHENES

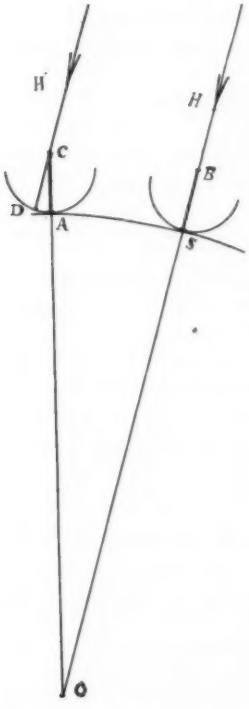
A translation from Cleomedes who gives the sole circumstantial account of the first measurement of the Earth's circumference as effected by Eratosthenes about 200 B.C.

SYENE and Alexandria, says Eratosthenes, lie under the same meridian. And since the meridians are the greatest circles in the universe, necessarily those that underlie them are the greatest circles of the earth. Accordingly, whatever size the method may prove for the circle of the earth through Syene and Alexandria, the same will be the size of the great circle of the earth. Also, he maintains, Syene lies under the circle of the summer solstice. Whenever then the sun, being in Cancer and making the summer tropics, comes exactly to mid-heaven, the gnomons of the horologes must become shadowless, the sun being perpendicularly above, precisely, and this becomes the rule over a diameter of three hundred (300) stadia.

In Alexandria, at the same hour, the gnomons of the horologes throw a shadow, inasmuch as this city lies more to the north than Syene. If now, in the meridian and the great circle of the cities, we draw a periphery from the end of the shadow of the gnomon in Alexandria upon the basin of the horologe itself, this periphery will become the graduation mark of the great circle among those in the bowl, since the shadow underlies a great circle.

If, thereafter, we imagine straight lines extended through the earth from each of the gnomons, these lines will meet at the centre of the earth. Since the horologe in Syene lies perpendicularly under the sun, we may perceive that the straight line coming from the sun to the summit of the gnomon becomes one straight line with that from the sun to the centre of the earth. And if we imagine another straight line drawn from the end of the shadow of the gnomon in Alexandria to the sun, this and the aforesaid straight line will be parallels, extending from different parts of the sun into different parts of the earth. Upon these lines, which are parallel,

falls the straight line coming from the centre of the earth to the gnomon in Alexandria so as to make the alternate angles equal. Of these angles, one is at the centre of the earth at the junction of the straight lines produced from the horologes to the centre, and one at the junction of the summit of the gnomon in Alexandria



with the line from the end of its shadow produced toward the sun through the tangency.

Now upon this latter angle was placed the periphery drawn from the end of the shadow and around the basin. Upon that at the centre of the earth lies the periphery from Syene to Alexandria. Now those peripheries are like which lie upon equal angles.

Whatever proportion then the arc in the scaphe holds to its complete circle, the arc from Syene to Alexandria holds the same. But the arc in the scaphe is found to be the one-fiftieth part of the whole circle. Necessarily, therefore, the interval from Syene to Alexandria is the fiftieth part of the great circle of the earth. And this is five thousand stadia. Hence the whole circle becomes twenty-five myriad (250,000) stadia.

And such is the method of Eratosthenes.

—*Translated by J. Weir.*

Translator's Notes:

1. The above literal translation is from the Greek of Cleomedes, I. 10., following the text of Dr. Hugo Berger's "Geog. Fragm. des Eratosthenes—neu gesammelt".

2. The fragment lacks the title and figure as supplied herewith. The argument is readily followed with this.

3. Syene (Assouan, near the First Cataract of the Nile) is in reality about 500 miles south and 200 miles east of Alexandria. It is sufficiently near the Tropic to justify Lucan in saying "*Umbras nusquam flectente Syene*".

4. The exact value of the length of the Greek stadion is disputed. If taken as 185 metres, Erastosthenes' result is 15% in excess.

5. For a discussion of above, reference may be made to Wright's "Geog. Lore of the time of the Crusades"—Amer. Geog. Soc. Pub., 1925.

6. It will be recalled that Eratosthenes of Alexandria, (240-196 B.C.), was the founder of scientific geography, and a contemporary of Archimedes. Except for references in other authors, his writings have not been preserved.

7. The scaphe was a hemispherical copper bowl with a vertical gnomon. Altitudes of the sun could be measured from the shadows cast. A system of hour circles and an equinoctial suited to the latitude allowed its use as a dial (horologion). See Marcus Capella VI, who also tells us that Eratosthenes learned the distance from Syene to Alexandria from the royal surveyors of Ptolemy.

8. Cleomedes' introductory remark in which he mentions a method of Posidonius, inferior in directness to this of Eratosthenes, is omitted here.

9. Dr. H. Berger's collection, "Geog. Fragm. des Eratosthenes" does not include translations. He edits this one as the sole fragment relating to Eratosthenes' Earth Measurement that has been preserved to us in precise statements without any defective flavour of wording.

10. The translator, whose knowledge of Greek is but indifferent, had constant recourse to the lexicon for the rendering. He believes the above is a literal translation, but has seen no other in any language.

RADIO TALKS OVER CFCF (VICTORIA, B.C.)

By W. E. Harper

XLII—Is There Life On Mars?
(March 3, 1931)

ALMOST directly above us, though a little to the south, are the two planets, Jupiter and Mars. Jupiter is the brighter and more westerly one; you cannot fail to recognize Mars, though, by its ruddy colour. I have already spoken concerning Jupiter, and the present seven minutes will be devoted to the much-discussed but ever-interesting question of the possibility of life on Mars.

Let us recall a few facts that we know about Mars. Naming the planets in order outward from the sun we have Mercury, Venus, the earth, Mars, Jupiter, Saturn, Uranus, Neptune, and the recently discovered Pluto. Mars is about one and one-half times as far from the sun as is our earth and consequently receiving only about one-quarter the light and heat that we do. It revolves about the sun in 687 days, and has its seasons of spring, summer, autumn and winter, much as we do, although they are correspondingly longer.

Mars has a diameter of 4,200 miles; it is thus the second smallest planet, being second to Mercury. Its mass is only one-tenth that of the earth. We would naturally not expect it to hold a very extensive atmosphere.

Its day is about 37 minutes longer than our own. Rotating thus on its axis we are able to see the different parts of its surface and it is decidedly interesting to observe the prominent features as they rise at one side and move across to the other. The poles of the planet are capped by large white areas discovered by Cassini in 1666. A century later Sir Wm. Herschel observed their variation in size with the progress of the Martian seasons. Near the end of the Martian winter, the polar caps are largest; as spring proceeds they gradually shrink in size, and frequently disappear alto-

gether towards the end of summer, but re-form again during the Martian autumn. As suggested a moment ago the atmosphere of Mars is very, very scanty. Some oxygen and water vapour are known to be present in it. There are no large oceans or seas on the surface of Mars, but many permanent darkish and greenish areas mottle its ruddy surface. In addition to these mottled areas a network of narrow straight markings of 1,200 miles average length overspread the equatorial and temperate zones of the planet. These are the canals. They were discovered by Schiaparelli in 1877 and were named by him "canali" or channels, which word rather unfortunately became translated into popular works as canals. Although at first doubted by some, their existence is established as they have been repeatedly seen and photographed.

There is unanimous agreement upon the foregoing facts; but not all agree upon the interpretation of the planetary markings. The general theory, as worked out in great detail by Percival Lowell, is that the polar caps are ice and snow, the dark areas are vegetation, the large red areas are deserts, the canals are strips of vegetation which have sprung up alongside the large central canal and its lateral ditches. Thus Lowell and his followers assume that there are beings of sufficient intelligence on the planet to engineer such canals as will carry the water from the melting polar ice caps and make use of it to irrigate the otherwise arid wastes of the equatorial region.

Not all astronomers hold such positive views as Lowell did. Cracks in the surface of the planet might well be mistaken for the narrower streaks, whilst the light reflected from the surface rock of different kinds might possibly account for some of the coloured areas. The main thought in the minds of objectors, though, is as to a sufficiency of water and oxygen and secondarily to the temperature conditions on the planet.

At recent oppositions of the planet, particularly those of 1924 and 1926, there have been increasing evidences secured of the presence of both water vapour and oxygen in sufficient amounts to sustain life on some scale at least. The extent of the atmosphere is now accepted as much greater than formerly, for photographs

made in infra-red light, which pierce below any atmosphere and record the actual surface itself, show considerably smaller diameters for the disc than those made in ordinary blue or ultra-violet light. These latter register the whole disk, atmosphere and all, and from the comparison we learn that an atmosphere, possibly 100 miles in depth, likely surrounds the planet.

Of equal importance are the determinations that have been carried out, at the Lick and Lowell observatories, of the temperature on the surface of the planet. Tiny electric thermometers, which are almost weird in their ability to record temperatures differing by a few-millionths of a degree, have told us that the temperature on the sunlit surface of Mars is comparable to that of a cool day in autumn on our own planet. With a scanty atmosphere the night temperature will rapidly fall, but no doubt through long ages human beings, if they do exist there, could accommodate themselves to much more rapid and extreme ranges of temperature than falls to our lot on the earth.

Thus, Mars, with some atmosphere and a slight amount of water vapour and oxygen, and with a surface temperature, which though cool, is not unbearable, holds out all the possibilities for life. Whether or not life actually exists there is another question; certainly, suggestions of radio communication in the present state of its development show a lack of appreciation of the problem.

XLIII—THE SOLAR SYSTEM MOVING THROUGH SPACE

(March 17, 1931)

MY topic concerns the motion of translation which the whole solar family as a unit is subject to. Without being aware of it we on the earth are experiencing a great variety of motions. The sphere on which we live is rotating on an axis, making one complete turn every 24 hours. Since the earth's circumference is about 24,000 miles, this means that a person at the equator would be moving 1,000 miles per hour, or slightly more than one-quarter of a mile per second. In our latitude the rotational speed would be

slightly less than at the equator, being in fact about one-fifth of a mile per second. We fail to experience any sense of motion, though, since the enveloping atmosphere, to a large extent, moves with the solid earth. It is much easier to believe that it is the sun, moon and stars that are drifting by in the opposite direction than it is to believe that our own earth is in motion.

If I may be allowed to digress just a moment, I would suggest to those who are interested in photography that they can easily obtain a rather interesting photograph which will show this rotation of the earth. The axis about which the earth is rotating, if produced outwards, would cut the celestial sphere in two points which are called the celestial poles. Right at the poles no motion is shown, but with increasing distance from it the circular sweeps of the stars become greater and greater. Adjust your camera, or an ordinary kodak, so that it points to the pole; that is to say, face it north and point it upwards about half a right angle. If there are no lights in the foreground you may safely leave it open for several hours on a starlit night. Upon development you will find portions of circles corresponding to the stars that have trailed across the plate during the period of exposure. Try it.

But beside this motion, due to rotation, we are also moving in an orbit about the sun as a centre. Our mean distance from the sun is about 93,000,000 miles, and as we complete the orbit in 365¼ days that means we must have a speed of about 18 miles per second. Even though this velocity of revolution is many times the preceding one due to rotation, we are never directly aware of it.

Then there is the motion which I want in particular to emphasize to-night. It is that of the sun and the whole family of planets, with their attendant satellites, journeying through space as a unit. If you were to consider that the motions of the planets about the sun could be represented on the top of a table this third motion would be equivalent to moving the table bodily in a definite direction and with a uniform speed. I shall try to show how we have become aware that such a motion of translation is taking place.

If you are seated in a railway carriage and looking out of the window as the train rushes along, the telegraph poles will seem to fly past in the opposite direction to that in which you are going.

The farther they are away from the track the less swift will be their apparent motion, but all objects as far as you can see them will appear to be receding to the rear. Something analogous to this should be experienced in the motions of the stars if our solar system is rushing through space. The stars on either side should appear to be moving back and away from the point in the heavens toward which we are heading.

Now while the stars are often spoken of as fixed they are fixed only in the sense that the naked eye cannot detect any apparent change in their relative positions, even after the lapse of centuries. Nevertheless, precise measures show that all the stars are in motion hither and thither, to us or from us, and across our line of sight. If our solar system were at rest relative to them there would probably be as many moving in one direction as in another and the fact that we find a preponderance of their motions directed *away* from a certain point in the heavens makes us feel fairly sure that we ourselves are the cause and that we are moving *towards* that apex. From a very limited number of such cross motions, Herschel, about 150 years ago, fixed the apex of the sun's way a little north and west of the bright star, Vega.

The measures which he made were all in angular measure, and not knowing the distances of the stars he was unable to convert such angular speeds into linear speeds. Since his day, however, a method of measuring the velocity of a star in the line of sight has been discovered. It was at once recognized that here was a method of testing the accuracy of the theory that our whole solar system was moving in the general direction of Vega. For, if it were so, then all the stars in the vicinity of the apex should on the average be apparently approaching us, whilst in the opposite part of the sky they should be, in general, receding.

It was found that the stars were behaving just as expected and definite confirmation was thus given to the theory of the motion of the solar system through space. The velocity determined for such motion of translation is about 12 miles per second. A year's journey at that rate is no small distance and one might expect the relative positions of the stars to show slight changes due to such cause.

XLIV—THE STRUCTURE OF THE SUN

(April 7, 1931)

IN a recent talk I told you something about the chemical composition of the sun and the various elements that go to make it up. We concluded that the materials in it were very much the same as those of which the earth is composed. The difference, of course, lies in the fact that, owing to the high temperature of the sun, the metals are in the form of metallic vapours rather than being in solid form as in the earth.

At that time I said little about the dimensions of the sun, and such call for a brief consideration now. As to the size itself we have learned in various ways that the sun's diameter is about 109 times the diameter of the earth or 864,100 miles. If we were to think of the earth as situated at the centre of the sun, then the moon, which is about 239,000 miles away from us, would be only a little over half way out to the sun's surface. We may never have fully realized the immense size of the sun till some such illustration is given. Another one I shall leave with you. If the earth were represented by a small pea, the moon could be thought of as a pin head one foot away, whilst the sun would be a globe two feet in diameter and about 220 feet distant. From the figures just given, we find that the sun's volume is about 1 1/3 million times that of the earth.

Its mass can be determined from the pull it exerts upon the earth, to keep it in the prescribed path. The mass is found to be 332,000 times that of the earth, and when we have regard to the relative sizes we deduce that the average density of our luminary is only about one-quarter that of our earth. Nevertheless, since the earth has an average density 5.6 times that of water we see that the sun is 40 per cent. denser than water. We should rather expect this average density to be much greater, since the enormous mass must exert a powerful pressure upon the materials near the centre and make them very compact. The explanation is that owing to the high temperature, most of the materials must be in the gaseous state. The temperature, I informed you on a former

occasion, is 6,000° C. at the surface and increasingly higher as we go towards the centre, where possibly it is reckoned in millions of degrees. Not even the hardest solid could withstand such temperatures; it would almost instantaneously be converted into gas. But it would not be gas as we know gases on the earth. The enormous pressures that exist in the interior of the sun must convert even gases into thick syrupy-like fluids. We can only infer that this is so; it is beyond our power to reproduce it.

The main disk of the sun, which is what we ordinarily see, is known as the photosphere. Over this is spread an atmosphere of vapours, relatively cooler, which is known as the reversing layer and which ear-marks for our researches all the light that passes through it. The depth of this layer is a paltry 500 miles or so. Over this again lies the chromosphere, another layer consisting of the lighter gases, particularly hydrogen, the spurting jets from which are often seen as brilliant scarlet prominences rising to heights of several hundred thousand miles. In addition to the lighter gases we have in the chromosphere the vapours of calcium, the atoms of which, though heavy, are very susceptible to the outward radiation pressure. Then at the time of a total solar eclipse, when the moon comes between us and the sun and blots out its strong light, we see besides these beautiful prominences a pearly-grey halo of light surrounding the whole disk, which is known as the corona. The corona extends outwards a million or more miles from the surface and changes its form from year to year. It probably may be considered as matter which is extremely rarefied and blown off by the intense light pressure from the sun itself. Thus we have the more or less solid portion of the sun, known as the photosphere, and the varied portions of the enveloping atmosphere, the latter having a mass infinitesimal as compared to the former.

The earth rotates on its axis once in 24 hours; the sun likewise, rotates on an axis, but requires on the average about 25 days. I say, on the average, because all parts of it do not rotate at the same rate; in other words it does not rotate as a solid body. The equatorial portions rotate faster than portions north or south of

the central line. That is to say, there is a lag as we go from the equator either way to the poles. A point half way from the equator to the pole takes two and one-half days longer to make one complete rotation than a point on the equator.

Our earliest notions about the sun's rotation came from observations of the sun spots, those large disturbances on the solar surface comparable to enormous volcanic eruptions. Into some of the openings produced by the outrushing vapours a dozen earths could be tossed without filling up the great gap torn in its surface. The rising vapours from the eruptions are cooled and show black against the intense white hot surface. Their motions can thus be easily followed from one side to the other as the sun turns on its axis and to an amateur must go the credit for being the first to discover, through such motions of the spots, the secret of the sun's rotation.

That the sun's rotation varies with the distance from the equatorial line is a fact now well attested by a variety of observations. We have come also to believe in another type of non-uniform rotation which cannot be tested out by observation, but which some of the leading theoretical astronomers like Jeans think to be the case. It is as if the sun were divided into several concentric shells with the outermost one rotating slowest of all and the innermost one the fastest, there being a gradual change from one to the other. If this be true, we cannot wonder that the surface of the sun is always in a state of flux as indeed we find it to be.

XLV—DO STARS COLLIDE?

(April 21, 1931)

THE subject announced may seem somewhat strange, but in the case of one small boy at least it pointed to his logical train of thought, following my address last month. You may recall that I spoke about the solar system rushing through space at twelve miles per second in the general direction of the bright star Vega. This thoughtful boy wants to know if we will hit Vega, and when.

Well, to put it in a nutshell, if we were going straight for the star and it stayed right in its present position, without doubt we

would collide with that stellar system. Ease your minds, however, for it would not be for hundreds of thousands of years, so great is the distance separating us.

Let us examine the conditions upon which will depend the likelihood of one star banging into another. As mentioned in these talks quite often, all the stars are in motion at random, but the problem before us is equivalent to considering them at rest and seeing what the possibility is of one star, travelling at an average speed, of making a hit on another star.

Naturally the bigger the target the better the chance of a hit. That leads us to consider the sizes of the stars. Briefly put, they vary from bodies one-quarter the diameter of our sun to enormous bulky stars like Antares, with a diameter 400 times as great as our sun. These latter giants would be the best targets, but as they are relatively scarce in comparison with the smaller variety, it would not be far astray to assume that the average star-target will be somewhat similar to our own sun with a diameter of the order of a million miles.

The second consideration will be the speed with which the star is going. If it is practically stationary it will not have many chances to come in contact with other stars, but if it is a high velocity star, speeding through space, the chances are better, in a given interval of time, for it to encounter others. Now the velocities of stars have been much studied the past twenty years or more, and we have a fairly good idea of their average speeds. Our sun at 12 miles per second is rather on the slow side, its space velocity being in fact just about one-half that found for the stars on the average. Our typical star will then be considered as having a space velocity of 25 miles or 40 kilometres per second.

The third consideration that enters into the argument is the density of the stars. The more thickly populated any region of space is the greater the chances of an encounter. From measurements of the actual distances of many of the stars nearest to our system, we can tell how many congregate in a given volume of space. The nearest one is 4.3 light years distant; Sirius, the bright one adorning the southwest sky in the early evening, is 8.8 light years

away, and within a radius of ten light years there are not over ten or a dozen stars known. If you were to consider a cube in the sky whose side is 200,000 times the distance from the earth to the sun, one star and one only, would be found in that great volume of space. Let me give an illustration, as bare figures fail to sink in. Think of the largest building in your city or locality. It may be the Empress Hotel or Vancouver or Olympic. If you were to consider a single small particle of dust requiring a microscope to detect it, to be floating around in the interior of such a building, such would be comparable to the isolation enjoyed by the stars in their spacious domain. You are beginning to see, I think, that the chances of two stars encountering each other are very small.

But there is another aspect which is more favourable. While no collision might occur for a century or a thousand years, such might happen if the interval of time ran into the millions or billions of years; a perfectly legitimate period in which such an operation might have been continuing. Again, we must consider not the chance of one particular star being in collision but any one of the billion known. Thus the chance that *any* star will be in collision is a billion times as great as in the case of *any one particular* star.

The problem is a perfectly straightforward one in mathematics and has been worked out, to show that even with ten billion stars actual collisions should happen only at intervals averaging about ten million years. The chances become correspondingly greater if, instead of having to make a direct hit, they be required only to approach within a definite distance, say like that of Saturn or Neptune from the sun. Some such close encounter occurred ages ago, it is thought, when the earth and other planets were pulled out of the sun by a passing star.

AXIAL ROTATIONS OF STARS*

THE widths of lines in stellar spectra are thought to be due to a variety of causes, among which, three may be singled out as of greatest importance. These are the abundance of the atoms producing the line under consideration, the so-called Stark effect and the axial rotation of the star.

As far back as 1877 Abney considered that the widths of lines would be a measure of the rotation, an idea which was ridiculed by Vogel because broad lines and narrow lines occurred in the same spectrum. The past decade has, however, seen actual measures of rotation made on eclipsing variables just before and after eclipse, and at Mount Wilson rotational speeds of a couple of hundred kilometres per second have been deduced. The contours of the spectral lines have recently been studied by many workers for evidences of rotation. In 1928 Carroll gave a mathematical treatment of the problem, developing formulae to relate the contour of the line to the effect of either rotation or expansion of the star. Earlier, in 1919, Shapley and Nicholson sought to find if the lines in Cepheid variables would, by their character, substantiate the pulsation theory. On this theory they should be periodically broadened first to the violet, then to the red, rather than suffer a direct bodily displacement as in the case of true orbital motion. They were forced to conclude that with the equipment of that day such differentiation could not be made.

It is a plausible assumption that in spectroscopic binaries of short period the period of rotation will be the same as the period of revolution. These short periods are naturally found among the so-called "earlier" types and among these, then, those with the shortest periods and largest orbital velocities will be most likely to show rotation. Observational evidence substantiating this seems to be found in the wide and shallow lines more commonly found in the "early" than in the "late" type stars. Rotation of the star

*Seminar D.A.O., No. 208. References: *Astrophysical Journal*, July, 1930, and preceding.

offers a satisfactory explanation. Small rotations cannot be detected from the contours of the lines themselves, possibly 50 km. per sec. being about the limit capable of definite determination. Shajn and Struve compared the character of lines in two spectra of Jupiter, one made with the slit of the spectroscope parallel to and the other with the slit at right angles to the equator. As there is a relative rotational velocity of about 25 km. between the two limbs, the former spectral lines should be broader and shallower, but such could be detected none too definitely.

Graphs were computed by Elvey showing the contours that lines would have for typical stars with rotational velocities of 70, 140, 210 and 280 km./sec. Using these as standards for comparison with the observed contours of 59 stars of types O to F, he found rotational velocities as high as 250 km./sec. (Altair). The mean of all 59 was 60 km./sec.

Struve found similarly for the brighter component of the spectroscopic binary Spica a rotational speed of 200 km./sec., although that of the fainter one does not exceed 50. The single star Eta Ursae Majoris, of similar type, has a contour identical to the former, hence he assumes it has a rotational velocity of 200 km./sec. also. Interesting speculations are made as to its stability.

W.E.H.

THE LUMINOSITIES AND PARALLAXES OF 350 STARS OF SPECTRAL TYPE B

By W. B. Rimmer*

MEMOIR No. 2 of the Commonwealth Solar Observatory, Canberra, Australia, is devoted by Rimmer to a discussion of the absolute magnitudes and corresponding parallaxes of 350 of the brighter B-type stars. For the most part these are of the southern stars, although a few entries are noted in the catalogue for stars of 30° and 40° north declination. Evidently they worked at low altitudes to obtain spectra of these stars.

*Seminar D.A.O. No. 212.

The objective prism equipment was a 9-inch refractor of 139 inches focal length, for which they secured a dense flint prism of 32° 40′ angle, the resulting dispersion being 52 mm. between Hβ and Hϵ. This is almost twice that given by our single prism at Victoria.

The method adopted to derive the absolute magnitudes was that of "line character" initiated by Adams and his colleagues at Mount Wilson and so successfully used by several other investigators. Increasing sharpness of the spectrum lines is shown to be synonymous with increasing real luminosities of the stars concerned. The stars were first classified into the various sub-types of the B class and then divided into five or six groups depending upon the sharpness of the spectrum lines. By means of cluster, group motion and parallactic motion parallaxes, empirical curves were built up connecting line sharpness with absolute magnitude.

Each sub-type is considered by itself so that there is not a rigorous standard of sharpness throughout all the sub-types, although the author says the variation is not large. A number of stars usually designated "peculiar" in the Draper classification required separate treatment. Such were stars with excessively sharp lines and stars showing broad absorpotion lines of hydrogen on which in some cases emission was superposed. His results in general confirmed those of Gerasimovic, that these last were more luminous than the average star of the type.

Cluster and group motion parallax data as derived by Kapteyn, Plummer and Rasmuson, were used to correlate luminosity with his "line character". To these parallaxes were added the results given by the parallactic motions. All the stars are listed in Boss's Catalogue, and corrections to the proper motions as given in the introduction to that work were first applied. It might be considered by some that Raymond's corrections should have been used. With a velocity of 20 km./sec. to an apex at 270° + 34° mean parallaxes for the various groups were deduced from the *tau* and *upsilon* components. Both the space and parallactic motions showed a correlation between "line character" and luminosity for each sub-type. Thus were his empirical curves established.

He finds in comparing his results with those of other observers no alarming differences. There are 170 of the stars common to his and Edwards' lists and slight differences can be accounted for by the fact that many of these are spectroscopic binaries for which Rimmer made corrections to the apparent magnitudes whilst Edwards did not do so. For 64 stars, Mount Wilson averages 0.4 magnitude fainter than Rimmer. Omitting four large negative trigonometric parallaxes there remain values for 65 stars in his list and there is good agreement between his magnitudes and those computed by the aid of these trigonometric values. The B3 sub-class are the most numerous, being 71 in number, and they range in absolute magnitude from –1.9 for the sharpest lines to –0.4 for the most nebulous.

There are 144 of the 350 stars previously classified as spectroscopic binaries, thirty per cent. of these showing the spectra of both components. Tabulating them according to absolute magnitude it is shown that there is a fairly uniform distribution of two-spectra binaries over the range 0^m to $-3.^m0$, the latter luminosity seeming to be the upper limit for binaries showing double lines. In the case of the single line binaries there seems to be an increase in the number of such as we proceed to the more luminous stars.

In Table I showing the relative strengths of lines as used in classifying the stars into sub-types there is evidently a slip, as the numbers in the second column should be the reciprocals of those shown. The paper is a valuable one and is the more welcome as coming from the southern hemisphere and in particular from the Commonwealth Observatory.

W.E.H.

A REMARKABLE SOLAR HALO

By F. H. Nicoll and R. N. H. Haslam

A HALO of unusual brilliance and completeness was observed at Saskatoon, Saskatchewan, Canada, between the hours of 8.15 and 9.30 (mountain time) on Thursday, April 16th, 1931. The maximum brilliance was attained at 8.45.

The weather conditions at the time were as follows:

Barometer (sea-level)	30.11 inches
Wind velocity	8 miles per hour (S.W.)
Temperature	49.5°F.
Clouds	Cirrus

When the halo was first observed the altitude of the sun was 35°.

Referring to the diagram herewith the symbols used denote:

S	Sun
HH	Horizon
Z	Zenith
aa	halo of 22°
bbb	portions of the halo of 46°
mm	parhelic circle
h	anthelion
ss	narrow oblique arcs of the anthelion
rr	apparently wide-angle oblique arcs of the anthelion
cc	upper tangent arc of the halo of 22°
c	lower tangent arc of the halo of 22°
F	a short arc above the halo of 22°, concave towards the sun
d	a short arc concave towards the zenith and tangent to the halo of 46°
EE	parhelia of 22°
E'E'	apparently secondary parhelia of 22°
PP	parhelia of 90°
P'P'	parhelia of 120°

White arcs are represented by two full lines, a full and a dotted line representing coloured arcs, the blue side of the arc being shown by the dotted line.

Previous observers have reported the wide-angle oblique arcs of the anthelion (rr) to be white, but definite colours were observed in this case.

Weather conditions have been favourable for the formation of

halos during this spring, as many have been observed throughout the Western provinces.

University of Saskatchewan,
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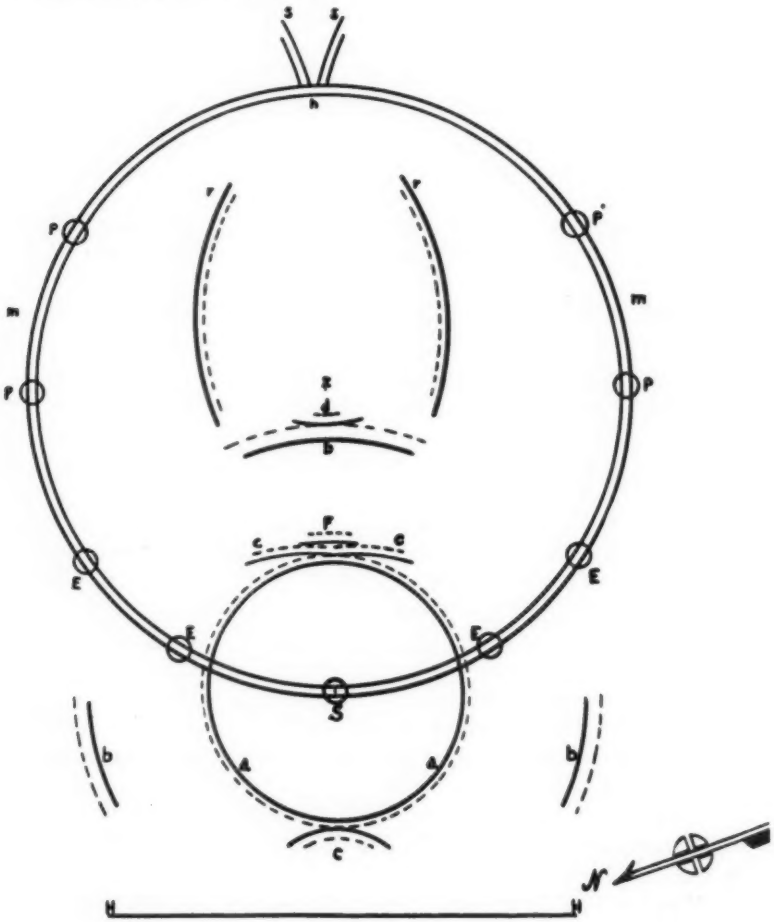

A Solar Halo seen in Saskatchewan, April 16, 1931

SEASONAL VARIATIONS IN ADOPTED CLOCK CORRECTIONS*

By C. C. Smith

THE meridian circle results at the Dominion Observatory give $\pm 0^s.013$ as the probable error of an observed clock correction derived from the values obtained by different observers on the same night; the residuals from a smoothed curve representing the adopted clock corrections give a probable error of $\pm 0^s.02$. It is probable that systematic errors increase this to approximately $\pm 0^s.03$. If the error of an adopted clock correction could be reduced to $\pm 0^s.01$, the following are among the many results that would be derived:

(1) The error of a measured right ascension would have approximately half its present value, or only one-quarter the number of observations would be required.

(2) Variations in longitude, not due to deviations of the earth's axis but to causes more local in nature and similar to those movements of the earth's crust which give rise to the Kimura term in latitude variations, could be observed.

(3) Theories of continental drift could be investigated much more readily than at present.

(4) The effect of the moon on observed clock corrections could be more easily proved or disproved.

(5) The present discordances in time and longitude determinations would largely vanish.

The adopted clock correction for any day is computed from observations preceding and following, as well as from the observations on that day. Paris and Tokyo plot the corrections and draw a smooth curve best satisfying the points. Washington and Ottawa weight the preceding and following observations inversely as the number of days in the interval. Greenwich adopts the corrections from a series in powers of t. In any such method of smoothing, with a good system of clocks under constant temperature and pressure, the probable error of the *adopted* clock correction should

*Read at the joint meeting of the R.S.C. and the R.A.S.C., Toronto, May, 1931.

be about one-half that of an *observed* clock correction. That is, the accidental errors in the adopted clock corrections should be reduced to about $\pm 0^s.01$. There remain, however, comparatively large periodic errors of some weeks' duration and smaller errors with a yearly period.

Naturally, in such determinations of time, the usual precautions are taken to avoid, so far as possible, the effects of systematic errors. Small transits are used as a check on collimation and clamp errors. Relays are carefully measured for lag. Primary clock systems usually comprise three or more clocks under constant temperature and pressure. Underground azimuth marks smooth out the errors in observations of azimuth. Short period terms of nutation are included for clock correction and excluded for rate and allowance made for the effect of the deviation of the pole.

Since 1918 daily reception of all the available wireless signals from those time sending stations, of which the time signals appeared useful for purposes of time comparisons, has been made by the Dominion Observatory. As an example, the last beat of the Rugby 18-hour signal on November 2, 1929, is noted thus:

Ottawa clock time of reception	$15^h43^m07^s.996$
Adopted clock correction	$+04.573$
Adopted longitude	$5^h02^m51^s.940$
Assumed transmission time	-0.018
Corrected time reduced to Greenwich	$20^h46^m04^s.491$

This time would be the same for all observatories were it not for the many small errors that enter. These are chiefly uncorrected instrumental, clock, and relay errors, lag in reception of time signals lateral refraction, and errors in adopted star positions, adopted longitude and transmission time. From similar results for different observatories a mean can be taken which will represent the nearest approach to "correct time" that is obtainable.

In the following table is shown a sample of the times of reception (the hours and minutes being omitted) at the observatories at Paris, Tokyo, Ottawa and Washington, together with the means of the times and the residuals for the four observatories.

TIMES DEDUCED AT FOUR OBSERVATORIES

RUGBY 18-HOUR SIGNALS

Date, 1929	Observatories					Residuals			
	Paris	Tokyo	Ottawa	Washington	Means	P	T	O	W
November 2	04.479	04.494	04.491	04.499	04.491	−.012	+.003	.000	+.009
3	01.052	01.104	01.057	01.063	01.069	−.017	+.035	−.012	−.006
4	57.613	57.615	57.609	57.622	57.615	−.002	.000	−.006	+.007
5	54.174	54.170	54.182	54.177	54.176	−.002	−.006	+.006	+.001
6	50.751	50.738	50.750	50.756	50.749	+.002	−.009	+.001	+.007
7	47.328	47.321	47.311	47.299	47.315	+.013	+.006	−.004	−.016
8	43.877	43.853	43.854	43.849	43.858	+.019	−.005	−.004	−.009
9	40.436	40.453	40.407	40.429	40.431	+.005	+.022	−.024	−.002

Since the systematic or periodic errors will, in general, be different for different observatories, and in the mean of several observatories should be small, the residuals for any observatory afford a fair criterion of the errors in its adopted clock corrections.

In time comparisons for precise longitude and similar measurements, it is important to know the prevalence of these errors in

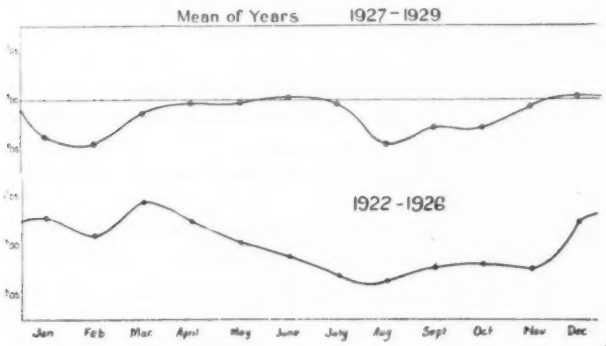

FIG. 1. Comparison of Ottawa Time with means of:
(a) Lower curve—Ottawa, Paris, Washington and Hamburg.
(b) Upper curve—Ottawa, Paris, Washington, Hamburg, Greenwich and Tokyo.

order that they may be avoided in so far as possible. For this purpose the monthly means of such residuals have been plotted on a vertical scale of $0^s.05$ to an inch.

In the comparison of Ottawa with the means of Ottawa, Paris, Washington and Hamburg for the years 1922 to 1926, there is evidently a decided yearly variation with low spots (Ottawa clocks too slow) in February and August. In the upper curve the monthly means of the residuals from the comparison of Ottawa clocks with the means of Ottawa, Paris, Washington, Hamburg, Greenwich, and Tokyo for the years 1927 to 1929 inclusive, are plotted. The yearly variation shown in this curve is, of course, different from the previous curve but it shows the same peculiarities of the Ottawa clocks—too slow by approximately $0^s.02$ in February and August. (Fig. 1).

In order to show that these peculiarities are not confined to Ottawa, but are also prominent in other observatories, similar curves have been formed for Washington, Hamburg, Greenwich and Paris, from the results of their comparison with the mean of nine observatories not including Ottawa. (Fig. 2). These results are published in the Bulletin Horaire, and are for 1929 only. Disregarding the dip in the Hamburg curve in May, for which there is evidently some other explanation, a smooth curve drawn through these points shows invariably a yearly curve not attributable to chance.

The amplitude of the variation, which is usually less than $0^s.05$, evidently differs for different observatories, and it is not certain that it recurs regularly in the time of any single observatory, though in the results of the wireless comparisons for the Dominion Observatory, for which the longest series of results are here available, that seems to be the case.

The conclusions suggested by the presence of these variations are that the different observatories should:—

(1) Continue the present investigations in level and azimuth errors and their cure.

Look for more data on the time of transmission of wireless waves and lag of reception.

Examine meteorological or other causes of error in determining clock correction.

FIG. 2. Comparison of Paris, Greenwich and Washington with Mean of Nine Observatories, 1929.

(2) Improve the primary sidereal clock equipment. The scheme proposed by the Bureau International de l'Heure is to have all observatories send their times of reception th the Bureau. The mean of all such times would be considered as time of a world clock from a comparison with which the different observatories would get the final corrections to their daily adopted times, The great practical advantages of this scheme are quite apparent. With clocks good enough the observed clock corrections can be smoothed by a series in powers of t, covering several months—Greenwich is hopeful of extending its formula over a year or so. The Ottawa investiga-

tions have been directed at exposing errors and searching for their cause, rather than in smoothing them out.

(3) Give the greatest weight to the continuous series of observations of time and time signals in investigations of accurate longitude and its changes.

Dominion Observatory, Ottawa,
May, 1931

REVIEW OF PUBLICATIONS

Measures of Double Stars, by Francis Preserved Leavenworth. viii, 118 pages, 9 × 11½ inches, with 1 plate. Publications of the Astronomical Observatory, University of Minnesota, Vol. 1.

The above publication issued under the direction of Professor C. C. Crump, Professor of Astronomy, of the University of Minnesota, contains a complete record of all measures of double stars by the late Professor Francis Preserved Leavenworth. These measures were made by Professor Leavenworth during the period from 1885 to 1927 inclusive, the telescopes used being the 40-inch and 20-inch of the Yerkes Observatory, the 26-inch of the Leander McCormick Observatory, the 10-inch of the Haverford College Observatory, the 16-inch of the Goodsell Observatory and the 10½-inch of the University of Minnesota. A large majority of the measures were made with the last named telescope as Professor Leavenworth was Professor of Astronomy and Director of the Observatory at the University of Minnesota for thirty-five years.

The Burnham General Catalogue number is given for all stars as well as the name of the star; the position is given for 1880 and 1950 and the position angle and distance are given for each epoch together with the average deviation of all the observations from the mean. The arrangement of the observations together with the fact that it is such a complete summary of Professor Leavenworth's measures of double stars makes it a very valuable reference work.

The closing pages of the volume contain a list of the publications of Professor Leavenworth. While much of his time was given to double star measures he also made many position observations on comets and nebulae and made some stellar parallax determinations while at Haverford College Observatory. In 1918 his interest was aroused in variables by the appearance of Nova Aquilae No. 3 and during the remaining years he divided his observing time between double stars and variables.

Following the double star measures of Professor Leavenworth is an appendix containing the double star measures of Professor William O. Beal, a member of the staff of the Department of Astronomy of the University of Minnesota since 1913.

It can be truthfully said of both Professor Leavenworth and Professor Beal that while their observational work was of a high character their outstanding contributions were as teachers of the oldest of the pure sciences. They were loved by the hundreds of students who came under their influence and their contribution through the lives of the students is a memorial greater than anything that can be wrought by the hand of man.

R. M. M.

NEWS AND COMMENTS

President E. W. Brown of the American Astronomical Society has asked the following Committee to study the various problems connected with the occupation of observing sites for the solar eclipse of August 31, 1932, and to make such information available to eclipse parties: Professor Frederick Slocum, Director of the Van Vleck Observatory, as Chairman, Mr. R. Meldrum Stewart, Director, Dominion Observatory, and Mr. Odin Roberts of Boston. Mr. Stewart is taking a motor trip through Quebec, near the St. Lawrence River where the line of totality passes, and Messrs. Slocum and Roberts are similarly investigating the southern end of the path from the Maine coast up to the Canadian line in Vermont, possibly as far as the St. Lawrence. In addition to examination of possible sites, information concerning meteorology, hotel accommodation and transportation facilities is being secured.

Mr. A. H. Miller and Mr. G. P. Hatton of the Dominion Observatory have left for Thetford, Quebec to continue their magnetometric surveys over the boundaries of the serpentine belt con-

taining the asbestos and chromite deposits on which valuable information was secured last year. In this work, they have the co-operation of Dr. H. C. Cooke of the Geological Survey, Ottawa. Following this work, Messrs. Miller and Hatton will proceed to the Onakawana lignite field to investigate it with the magnetometer and torsion balance in co-operation with Messrs. Hawkins and Westaway of the Ontario Research Foundation.

A Canadian Biological Conference meets from July 24 to August 1, at the summer residence of Mr. Copley Amory at Matamek River, Saguenay County, Quebec. Mr. Amory has invited scientists from all over the world to confer regarding cycles in living things and climate. A contribution from the Dominion Observatory, Ottawa, prepared by Ralph E. De Lury and John L. O'Connor, will be presented by the former, and will deal particularly with the Canadian records of living things and meteorological elements in relation to possible sunspot and tidal influences.

The death is announced of M. E. Cossart, director of the observatory at Toulouse.

Two plates taken by Glen Moore with the 10-inch telescope at Mt. Wilson observatory on July 17, confirm Nagata's Comet at 10h. 41m., + 9° 48′, for 8h. 26m., Pacific Standard Time.

R. E. DeL.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

Personal Notes

Congratulations and best wishes for continued good health to Mr. W. E. Harper, of the Dominion Astrophysical Observatory, Victoria, who has completed twenty-five years in the Government's Astronomical Service; and to Professor Edwin B. Frost, Director of the Yerkes Observatory, who reached his sixty-fifth birthday on July 14th. Professor and Mrs. Frost and Miss Katharine Frost were "at home" to over three hundred friends on the afternoon of the above date. Professor Frost will retire shortly.

A Fire-ball seen at Prairie River, Saskatchewan

Mr. Norman E. Usher writes:—On July 2 a friend and myself were standing talking on the highway, and glancing south I saw what I first thought was an ordinary shooting star, but it was not, as it passed right across the sky going from south to north. I pointed this out to my friend and we watched it. When it was south-west of the above town it broke in two, and the smaller piece went ahead, slowly gaining on the larger piece. When first seen it had a long tail, but after it burst there was only a short broad tail. I wrote to the *Winnipeg Free Press*, asking if any reports on this phenomenon had been received but none had come in.

An Example of Ball Lightning

Ball lightning is one of the rarest and most mysterious of natural phenomena. It is certainly electrical in nature, but is entirely different from other lightning discharges. It floats through the air or moves along a wire or the ground. Every year there are tragic deaths from ordinary lightning strokes but no fatalities have been attributed to it.

An interesting case occurred at about 3.30 a.m. on July 20, 1931, at the home of Mr. Jack Wilkinson, 137 Sherwood Avenue, Toronto. The following account is in Mr. Wilknson's own words:

"While my wife was at the rear of the house closing the windows two balls of fire came in the open window which faces the

north. They were about two feet apart and came with a sizzling sound, 'and then some'. Owing to the suddenness with which they appeared and the speed with which they travelled it is impossible to say how large the balls were, but they were larger than walnuts.

There was a terrific peal of thunder. I was almost blinded. They seemed to land on the pillow beside me, but I believe they struck the bed light, causing a flash, also a short circuit which blew out a fuse. Particles of metal from the lamp which was struck, fell on the pillow-slips and burned several tiny holes in both of them. It looked at the time as if the pillows were both in flames. The lights in the front of the house were put out, although those at the rear were not affected.

I hope I don't see any more of these balls of fire!"

A Romance of Comet Hunting

Some of the most pleasing incidents in astronomical discovery relate to comets, and the lives of Messier, Pons, Swift, Brooks, Barnard, Perrine and others are full of inspiration for the amateur. The latest discovery of a comet was made by Masani Nagata, a Japanese head foreman on a ranch in the Imperial Valley in California. He is 45 years old and owns a three-inch telescope which he obtained a few months ago, replacing a two-inch instrument used previously by him. It will be interesting to give Mr. Nagata's own story, which he supplied to *Science News Letter*.

"I am purely an amateur astronomer. On July 15 about 8.30 p.m. Pacific Standard Time I was observing the planet Neptune with my three-inch telescope which has a thirty-diameter eyepiece and an altazimuth mounting. Near one of the stars in the constellation of Leo I found what seemed to be a nebulous hazy star. As I am very familiar with that area of the heavens I was therefore very puzzled and doubtful about what I had seen. Soon the hazy unfamiliar star faded away under the horizon.

The next evening at about 8 p,m.. observing the same point, I found the same "star" moved about one degree to the northeast. It seemed to be a comet. As I could not believe that a comet of this brilliance, magnitude seven, could have escaped the eyes of thousands of comet seekers with much more improved and more efficient instruments than mine, I inquired at Mt. Wilson Observatory if such a comet were known. They replied in the negative. Then I pointed out to them the position and . . . they answered me that two photographs they had taken showed the comet.

As I am only an amateur astronomer, I have had no special study or courses in astronomy in school. Perhaps I inherited my interest in the stars from

my father. I am head foreman for one of Sears Brothers, Inc., ranches near Brawley, Calif., which grow vegetables and melons.

If this discovery does anything for the good of astronomy, nothing will please me more.

Computations made at the Students' Observatory, University of California, Berkeley, show that the comet passed perihilion on June 15, at a distance of one hundred million miles. Photographs showed a tail four degrees long.

ANOTHER SIX-INCH REFLECTOR

Rev. Cyril E. Martin, of Major, Sask., sends an interesting account of a recently completed six-inch reflecting teelscope, with a focal length of 49 nches. He also enclosed photographs of the instrument, mounted in the snow near his church, which however we have not been able to reproduce.

The speculum was made from two discs of ordinary plate glass one inch thick, and by following instructions laid down in "Amateur Telescope Making" a book published by the Scientific American Publishing Company, I was able to produce a parabolized mirror. This part of the work proved very interesting, even though it took most of my spare time for almost two months. The Foucault knife-edge test was especially interesting, and was in itself well worth the trouble of grinding the mirror just to become familiar with one of the most sensitive scientific tests. When I first applied this test I found that there was a distinct crater in the centre of the mirror: this was taken out by cutting away part of the pitch tool. However I met no real difficulty in making the mirror until I came to the silvering. The first attempt, while not an absolute failure was next thing to it. It took almost half an hour for the silver coating to form and then it was so thin and soft that it was practically useless. Later I discovered that this was due to using too much ammonia in the silvering solution. The second attempt proved successful for a good thick hard coat was the result, so thick that the sun could not be seen through it.

The mounting was made mostly from parts of an old Ford car. The polar axis consisted of a rear axle fitted into the drive-shaft housing. On the end of the axle is a Ford rear hub and bolted to this is a Ford brake drum. The outside of the hub is threaded to screw into a 2″ pipe tee, which with two other pieces of 2″ pipe forms the declination axis housing. The declination axis is also a Ford rear axle with hub and break drum and onto this is bolted the cradle which carries the telescope tube. A 3 inch swivel bearing was clamped round the housing of the polar axis and this was firmly fixed to the base of an old cream separator and bolted to a concrete block.

The purpose of the brake drums is for setting circles. I made these out

of strips of aluminum which I marked off in degrees and bolted round the drums.

All the Ford parts used were from the old model "T" and can usually be had from any garage for the taking away.

I am very well satisfied with the results of this telescope. The moon provided a very interesting sight. So also do Jupiter with his four visible satellites; Venus, a number of double and other interesting types of stars.

I am going to try photographing the moon and would be pleased to hear from anyone who has had success in that line.

MR. MILLER'S TELESCOPE

In the last number of the *Journal* was an announcement that the telescope of Mr. A. F. Miller, with accessories and dome, is for sale. Mr. Miller was a charter member of the Royal Astronomical Society and long before it was organized he was active in astronomical work. Now, with advancing years and impaired eyesight, he is unable to use his fine instrument. Mr. Miller knew well how to employ his great ability and his excellent equipment and the telescope is still in condition as new. Inquiries are to be addressed to Charles Potter, Optician, 16 Adelaide St. W., Toronto.

CORRECTIONS TO SKETCH OF SIR CHARLES PARSONS

The present writer wishes to make three small corrections to his sketch of Sir Charles Parsons which appeared earlier in this volume (p. 185). He tried to be accurate in every detail but was not able to refer to original sources and had to depend on authorities which were not absolutely without error.

Sir Charles was the sixth son of the third Earl of Rosse. The second and third sons died in early boyhood, and of the four who grew to manhood he was the youngest. He left Cambridge University in 1877. In referring to the steam carriage constructed by Charles and his next elder brother it was stated that there was an accident to a pedestrian. This was in 1869. As a matter of fact, Lady Bangor, a cousin of the family, who was riding on it, fell off and was instantly killed.

The above corrections are made from an admirable obituary notice in the *Proceedings of the Royal Society*, prepared by "J.A.E." (Sir James Ewing), who is thoroughly acquainted with the scientific work of Parsons and was a personal friend since 1891.

C.A.C.

MEETINGS OF THE SOCIETY

At Ottawa

On Friday evening, March 27th, the Ottawa Centre held an open meeting in the Victoria Memorial Museum. The Hon. Pres., Mr. C. C. Smith, was in the chair.

The title of the lecture was "Methods of Geophysical Prospecting", by Dr. Lachlan Gilchrist, Prof. of Physics, University of Toronto, and Mr. A. H. Miller, M.A., of the Dominion Observatory.

In view of the importance of geophysical prospecting in the locating of minerals, the Dominion Government has during recent years carried on a number of surveys and investigations throughout the country for the purpose of determining the applicability in Canada of the various physical methods and also for the purpose of assisting in their development.

In connection with these investigations Dr. Gilchrist and Mr. Miller made several surveys in Ontario and Quebec last year. In these surveys they made use of electrical, magnetic and gravitational methods.

Dr. Gilchrist spoke first on the results of electrical measurements of the conductivity of the earth in the neighbourhood of several deposits of minerals. "The conductivities of pyrite and pyrrhotite were much less than that of the surrounding materials and these deposits were readily delineated by the field measurements. The conductivity of lignite as obtained by laboratory measurements was only four or five times that of the surrounding materials. The results of the field measurements showed that the depth of overburden could be determined with a fair degree of accuracy in many places. The conductivity of chromite was in general lower than that of the surrounding dunite which was much affected by the presence of water, as these materials had a lower conductivity than water. While the results of the measurements gave a general indication of the presence of the chromite deposit it would appear that the presence of water made it very difficult to obtain definiteness of delineation by the electrical methods which were used in the investigation."

This was followed by a talk from Mr. Miller on the magnetic and gravitational methods. "For the principle of the simple torsion balance, that is to say, a beam weighted at the ends, we are indebted to the Rev. John Michell who planned the Cavendish experiment, and to Coulomb who designed independently a torsion balance for use in his electrical experiments towards the close of the eighteenth century. Notwithstanding this, full credit is due to the great Hungarian scientist Baron Roland von Eötvös (1848-1919) who is responsible for both the development of the theory in connection with the torsion balance known by his name and also for the practical development of an instrument that would measure the theoretical effects".

"An explanation was given of the quantities the torsion balance measures and also an account of the results that have been obtained in the investigations that have been made in eastern Canada during the last two years."

"Surveys of three faults in the vicinity of Ottawa where the geological conditions differed considerably, have been made and also of a large pyrite deposit. In every case it was possible to obtain information of value from the results even in the case of the pyrite deposit where owing to the topography and other circumstances the survey was rendered especially difficult."

"Several surveys were also made with the magnetometer but as a paper will probably soon appear in the Society's JOURNAL dealing with these results and those on hand wiith the torsion balance, it is not considered necessary to make any further reference to these here."

At the close of the meeting, Mr. Smith heartily thanked the two speakers for their interesting talks.

M. S. BURLAND

AT TORONTO

April 14, 1931.—A regular meeting was held in the Physics Building of the University at 8 p.m. Mr. R. A. Gray in the chair. There were three elected to membership in the Society:

Rev. H. A. Meek, 333 Harbord St., Toronto
A. E. Coleman, 1627 Bloor St. W., Toronto..
M. L. Bergara, Esperanza 3615, Buenos Aires, Argentine Rep.

The lecture of the evening was by Prof. Samuel Beatty, Ph.D., on "Some Mathematical Paradoxes". The lecturer stated that paradoxes in mathematics serve a useful purpose in calling for a revision of the logical argument or its basis. He called attention to several of these which have assisted in the development of mathematics at critical times in its history. He stated that in most paradoxes the notion of infinity is not carefully enough defined.

The Chairman expressed his pleasure at hearing Dr. Beatty's lecture and added some simple paradoxical examples.

Mr. J. R. Collins described the constellation Draco and its position.

April 28, 1931.—A regular meeting was held at the usual place and time, Mr. R. A. Gray in the chair.

There were seven elections to membership:

E. H. Carson, 113 Davisville Avenue, Toronto
R. Coughlin, 4082 Euclid Avenue, Detroit, Mich.
F. W. Woods, M.D., 109 S. Cordova St., Alhambra, Calif.
Ford. E. Bridges, 302 Philcade, Tulsa, Okla.
F. G. de Wolfe, Vernon, B.C.
James Stokley, B.Sc., M.A., The University Club, Philadelphia, Pa.
C. B. Parker, M.D., Medical Arts Bldg., Toronto

The constellation study "Virgo" was ably given by Mr. J. R. Collins.

The speaker for the evening was Prof. H. H. Plaskett, M.A., of Harvard Observatory, who selected as his subject "The Nebulae outside the Galaxy". The lecturer commenced by comparing the progress and revolutionary developments in physics during the past fifteen years with the comparatively meagre progress in astronomy. He hazarded the opinion that, apart from the intrinsic difficulties of astronomy due to the fact that experiments were not possible, much of the lack of astronomical progress could be traced back to the uncritical attitude of astronomers. He then proceeded to illustrate this thesis by a consideration of the physical nature of the extra galactic nebulae. The fundamental difficulty in this case is met in the attempt to determine the distance of these objects. After describing in detail Hubble's method of using Cepheid variables, the lecturer pointed out some of the difficulties that this method involved and finally concluded that, possibly on account of the absorption of light in space, the distances so assigned were too great. Knowing the distance, it was possible to learn something of the physical nature of these extra galactic objects. Considerable attention was given to the theoretical investigations of ten Bruggencate, which showed the elliptical nebulae to be composed of clouds of dust or molecules illuminated by light from some enormous central star. Evidence for the existence of similar gaseous clouds in the spiral nebulae was also referred to. The lecturer concluded that the extra galactic nebulae were probably not island universes, but were rather manifestations of what nature could accomplish with huge masses of gas in those regions of space remote from the disturbing influence of the galactic system.

S. C. Brown, *Recorder.*

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The Astronomical and Physical Society of Toronto was incorporated in 1890, though it had existed some years before this; in 1900 it became The Toronto Astronomical Society; and on March 3, 1903, it was given permission to use the name, The Royal Astronomical Society of Canada.

Its objects are to study astronomy and cognate subjects, to publish the results of its work, and to maintain a library.

For many years the Toronto organization existed alone, but now the Society is national in extent, having active Centres in Montreal, P.Q.; Ottawa, Toronto, Hamilton and London, Ont.; Winnipeg, Man.; and Victoria, B.C. Among its 800 members are a number of leading astronomers and scientists of the world, many amateurs, and, in addition, many laymen who are interested in the culture of the science.

The Society publishes a monthly JOURNAL containing each year about 500 pages of interesting articles and a yearly OBSERVER'S HANDBOOK of about 80 pages, containing valuable information for the amateur observer. Single copies of the JOURNAL or HANDBOOK are 25 cents.

Membership in the Society is open to anyone interested in astronomy. Annual dues, $2.00; life membership, $25.00 (no further dues). Publications are free to the members, or may be subscribed for separately.

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The library and offices of the Society are at 198 College St., Toronto, Ont. Applications for membership or for further information should be addressed to a local secretary (see back of this page) or to the General Secretary at the above address.